December 10, 1896.

Sir JOSEPH LISTER, Bart., F.R.C.S., D.C.L., President, in the Chair.

A List of the Presents received was laid on the table, and thanks ordered for them.

The President announced that he had appointed as Vice-Presidents—

The Treasurer.
Professor Clifton.
Mr. Thiselton Dyer.
Dr. Huggins.

The following Papers were read:—

I. "On Professor Hermann's Theory of the Capillary Electrometer." By GEORGE J. BURCH, M.A. Communicated by Professor BURDON SANDERSON, F.R.S.

II. "An Attempt to determine the Adiabatic Relations of Ethyl Oxide." By E. P. PERMAN, D.Sc., W. RAMSAY, Ph.D., F.R.S., and J. ROSE-INNES, M.A., B.Sc.

III. "The Chemical and Physiological Reactions of certain Synthesised Proteid-like Substances.—Preliminary Communication." By JOHN W. PICKERING, D.Sc. (Lond.). Communicated by Professor HALLIBURTON, F.R.S.

IV. "An Experimental Examination into the Growth of the Blastoderm of the Chick." By RICHARD ASSHETON, M.A. Communicated by ADAM SEDGWICK, F.R.S.

"On Professor Hermann's Theory of the Capillary Electrometer." By GEORGE J. BURCH, M.A. Communicated by Professor BURDON SANDERSON, F.R.S. Received July 21,—Read December 10, 1896.

I have received, by the courtesy of Professor Hermann, a copy of his paper* on "Das Capillar-Electrometer und die Actionsströme des

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* 'Archiv für die Ges. Physiologie,' vol. 63, p. 440.

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Muskels," in which he discusses the analyses of certain electrometer curves of muscle variation described by Professor Burdon Sanderson.*

His first statement demands an explanation on my part. He says, "Bevor ich auf Sanderson's Versuche und Schlüsse eingehe, möchte ich zeigen dass der von Burch und von Einthoven aufgestellte, das Capillar-Electrometer betreffende Satz, welcher der Construction zu Grunde liegt, auch aus meiner Theorie des Instruments unmittelbar folgt, was beide Autoren, obwohl sie meine Arbeit erwähnen, nicht bemerkt haben. Da beide ihren Satz *empirisch* gewonnen haben, so kann derselbe als eine schöne Bestätigung meiner Theorie betrachtet werden."

As a matter of fact, I did not know of Professor Hermann's paper until after I had formed my own theory. In my second paper† on the subject I mentioned that it had also been treated by him, "mainly from a mathematical standpoint," and implied that, in my opinion, his data were insufficient. I still think so, and cannot admit that my experimental results prove the correctness of his views.

That a mathematical formula, based upon a certain hypothesis, should agree with observed facts may be strong evidence in its favour, but is not necessarily a proof of the soundness of the hypothesis.

For instance, the equation

$$p = \mathrm{E} \,.\, e^{-rt}$$

may represent the discharge of a Leyden jar through a circuit of no inductance, or the swing of a pendulum in treacle. That it happens to be also the expression for the time-relations of the capillary electrometer does not of itself imply that the same causes are at work in all three cases, but simply that the forces concerned are so related that the movement is dead-beat. Professor Hermann, starting from Lippmann's polarisation theory, assumes the simplest conceivable relation between the rate of polarisation and the acting P.D., namely, that they are proportional to one another. Putting i = the intensity of the current, and p = the amount of polarisation at the time t, he gets

$$dp/dt = hi,$$

in which h is an instrumental constant.

Writing E for an electromotive force, which may be constant or variable, and w for the resistance of the circuit, he arrives at the differential equation

$$\frac{dp}{dt} = \frac{h}{w}(\mathrm{E} - p).$$

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* 'Journal of Physiology,' vol. 18, p. 117.

† "Time-Relations of the Capillary Electrometer," 'Phil. Trans.,' A, vol. 183, p. 81, 1892.

My position in relation to the problem was very different. I wanted to make a capillary electrometer from the description given in Lippmann's Theses. In order to get better results, I determined by actual experiment what were the conditions of sensitiveness and rapidity, and in doing this found out so much about the instrument that the "einfachste denkbare Annahme," referred to by Hermann, would not have commended itself to me.

My paper on the "Time-Relations of the Capillary Electrometer" was a condensed account of a small portion of the work done by me. For various reasons I did not then enter into my views as to the theory of the instrument, and will confine myself here to a statement of them, which must be regarded as preliminary.

Professor Hermann speaks of my theory having been empirically obtained. I demur to that expression as open to misconstruction. My working formula may rightly be called empirical, since it neglects certain terms of the complete expression, which I have found to neutralise each other in a suitably selected instrument, but my theory of the time-relations of the capillary electrometer was founded upon first principles and verified by experiments.

My starting point was the fundamental fact that in the capillary electrometer a mechanical effect is produced by an electrical cause. But there are several links between the cause and the effect, and a strong probability that each of them involves a time-function.

They are shown in the following scheme:—

I.	II.	III.	IV.
A difference of potential (the establishment of which is delayed by the (varying) internal ohmic resistance of the electrometer) produces	A change in the constant of capillarity at two interfaces between mercury and an electrolyte.	Presumably giving rise to polarisation at the aforesaid interfaces.	And does work in moving a column of mercury against the force of gravity (with more or less rapidity according to the (varying) amount of fluid friction in the tube).

Poiseuille showed in 1846 that the flow of a liquid through a capillary tube varies directly as the pressure. Of this I was not aware till later, but it leads to precisely the same differential equation as that adopted by Hermann.

Writing Q for the quantity of electricity, C for the constant of capillarity, P for polarisation, and W for the work done, the symbolical expression of the problem is—

$$f(Q_t, C_t, P_t) = \phi(W_t).$$

Hermann has passed over C, and omitted to take W into account, confining himself to the theoretical relation between Q_t and P_t.

But we know very little about polarisation, save in the case of solid electrodes. The term polarisation, as frequently used, includes two phenomena, which ought to be kept distinct, viz.:—

(*a*) That condition of the interface between two conductors, of which one at least is an electrolyte, in which the molecules are under a stress not greater than they are capable of supporting without chemical change.

(*b*) A deposit upon the surface of a solid, or in the contiguous liquid, of the products of actual electrolysis.

If one of the conductors is a solid, the inevitable local differences of condition or of composition enable actual electrolysis to take place, even with a P.D. smaller than that proper to the chemical change implied.

But if both conductors are liquid and perfectly pure, the stress is so far equalised that no electrolysis is possible until the E.M.F. reaches a certain value, more sharply defined in proportion as the materials are pure.

I hold that with differences of potential which do not reach this limit, the electromotive force is transmuted without electrolysis into mechanical force, and manifests itself as kinetic energy, until by the motion of the meniscus it becomes transformed into potential energy.

The locus of transformation from electrical to mechanical force must clearly be the two interfaces mercury-acid and acid-mercury, and it is upon these that the stress acts. The resistance is distributed along the tube, and is partly electrical, but to a far larger extent mechanical.

Is it reasonable, therefore, to assume that the sole cause of delay is the "Polarisations-geschwindigkeit" of the meniscus?

I believe that in the case of an interface between two liquids, the rate of polarisation is to be measured in terms of the vibration-period of a molecule, rather than in decimals of a second.

Actual electrolysis is another matter, and I hold that it does not take place in a properly working electrometer. We do not assume electrolysis when two pith balls repel each other after receiving a charge, nor when a closed coil is slipped over a magnet. But the coil cannot be got off again, nor can the balls fall together without the generation somewhere of a current. I cannot see why we should assume electrolysis in the case of the capillary electrometer. The marvellous rapidity of the action to which I have not yet found a limit, is against it, as is also the fact that the substitution for the acid, or the addition to it, of any substance which can be electrolysed by a smaller electromotive force, reduces the range of potential difference for which it can be used.

The presence of even a trace of impurity is soon manifested by the blocking of the capillary, and if this block is removed by electrolysis, the instrument behaves for some time in an abnormal way. It shows signs of a residual charge, like that of a Leyden jar, the mercury rising again after the short-circuiting key is opened, instead of simply ceasing to fall.

This I ascribe to polarisation of the kind met with between solids and electrolytes, and to this the term "Polarisations-geschwindigkeit" would be applicable. But no good electrometer will show it, except with electromotive forces greater than ought to be employed. I have held from the first that the capillary electrometer acts by transforming electrical into mechanical energy without any chemical interchange, and that this is possible because at the interface between two liquids which do not diffuse into each other the stress is so evenly distributed that no one molecule can be strained to a degree sufficient to detach any part of it until the stress is intense enough to break down all similar molecules simultaneously.

But if by polarisation is meant this condition of the interface, then I maintain that it must precede the movement, and must be developed with almost inconceivable rapidity.

In order to investigate the form of curve produced by recording the motion of the meniscus when the electrometer is acted upon by an electromotive force varying with the time according to some known law, *e.g.*, the pulsating or alternating current of a dynamo, Professor Hermann puts his equation into a somewhat different form, namely:

$$dp/dt + rp - r\epsilon f(t) = 0,$$

where r and ϵ are constants, and $\epsilon f(t) = \mathrm{E}$ is the electromotive force represented as a function of the time.

But this is simply my own formula for the estimation of the E.M.F. expressed as a differential equation.

For dp/dt is, in the polar curves taken with my machine, merely the subnormal N, and rp is identical with $k\Delta r$, whence

$$dp/dt \qquad + \qquad rp \qquad = \qquad r\epsilon f(t)$$

is identical with

$$\mathrm{N} \qquad + \qquad k\Delta r \qquad = \qquad \frac{1}{0{\cdot}0133} \qquad f(t) \text{ volt.}$$

which being interpreted signifies

$$\left\{\begin{array}{c}\text{The sub-}\\ \text{normal}\\ \text{to the}\\ \text{curve.}\end{array}\right\} + \left\{\begin{array}{c}\text{A constant mul-}\\ \text{tiple of the dis-}\\ \text{tance from the}\\ \text{zero-line.}\end{array}\right\} = \left\{\begin{array}{c}\text{A constant}\\ \text{multiple of}\end{array}\right\} \left\{\begin{array}{c}\text{The}\\ \text{E.M.F. at}\\ \text{time } t \text{ (in}\\ \text{volts).}\end{array}\right\}$$

Professor Hermann finds the complete primitive of this differential equation, and then, introducing various values of r and the function $E = \epsilon f(t)$, draws, by a process which is indeed laborious, the curves of the corresponding excursions. My own method gives a good deal of the information so obtained in a much simpler manner.

Adopting the letters used by him, when f vanishes we have

$$dp/dt + rp = 0,$$

that is to say, whenever the E.M.F. falls to zero the reduced values of the subnormal and the radius vector are equal, but of opposite sign, and the curve, therefore, can never come back to the zero line under the action of a current which pulsates but does not alternate (see figs. 2 and 4 in Hermann's paper). When the meniscus crosses the zero line, $rp = 0$, and $dp/dt = r\epsilon f(t)$, *i.e.*, the impressed E.M.F. is then directly proportional to the subnormal. This involves the further fact that the crossing of the zero line by the meniscus must always lag behind the change of sign of the E.M.F.

If dp/dt vanishes, as it does at the apex of a spike or the bottom of a notch, the instantaneous value of the impressed E.M.F. is directly proportional to the distance of the meniscus from zero.

The curves drawn by Professor Hermann are for the most part, so far as the eye can judge, similar to those obtainable under like conditions with the capillary electrometer. I have photographed and analysed many such, using rheotomes and dynamos of various kinds, both alternating and direct current, as sources of E.M.F. I have proposed, in a paper which has been in the publisher's hands since last November, that this method should be used to determine the characteristic current curves of dynamos.*

All the confusing influence of the lag vanishes when such curves are analysed—there is no need to trouble about the equation to the curve, since each several term of its differential equation at any given point is found at once by my mode of analysis. But I must point out that an error has crept into Professor Hermann's rendering of the curve given in fig. 6—or, rather, as it only pretends to be an approximation, that it is not equally accurate throughout. The portion $c'd'$, which corresponds to a diminishing negative (below zero) potential is represented as rising with increasing velocity instead of falling more slowly, as it should do. Yet, when this negative potential ceases, the curve commences to fall from d' to e' along the logarithmic curve of discharge. This is impossible. When $\epsilon f(t)$ is negative, the algebraic sum of dp/dt and rp must be negative also if the fundamental equation holds good. Probably the straight line cd has been placed too far to the right.

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* 'The Electrician,' July 17, 1896, *et seq.*

Professor Hermann questions the accuracy of my method of analysis when applied to steep curves.

My answer is that I do not employ it in such cases, preferring to take photographs of sudden changes upon plates moving with sufficient rapidity to suitably develope the curves.

Thus in Professor Burdon Sanderson's paper,* figs. 1, 2, 3, and 4, on Plate 1, and figs. 3, 4, and 5, on Plate 3, were intended to show within the limits of a page the entire course of certain phenomena. I did not analyse them, but simply measured the times of the maxima and minima. The remaining curves, viz., figs. 5 and 6, Plate 1, figs. 1—7 on Plate 2, and figs. 1 and 2 on Plate 3, are all suitable for analysis, with the exception of the first phase of fig. 1, which is almost too steep. I have done some thirty or forty of this kind.

As regards the further criticisms, so far as the physical interpretation of the curves is concerned, I can only say that cases did occur in which the maximum E.M.F. of the second (positive) phase exceeded the maximum E.M.F. of the first (negative) phase of the same response. With respect to curves, like those in figs. 3 and 4, Plate II, the part referred to by Professor Burdon Sanderson as the "hump," is not merely the curve of discharge. The actual negatives which I measured show a *rise* of the meniscus after its rapid downward movement has ceased, and *while it is still above the zero line*, and a similar rise is plainly visible to the eye after every one of the "spikes" in figs. 1, 2, and 4, Plate 1, which were photographed with the machine moving more slowly.

It is impossible for the mercury, under these conditions, after approaching the zero line, to recede from, without crossing it, except under the influence of a *negative Acting P.D.* That is to say, the Impressed E.M.F. must be of the same sign as the charge already in the instrument, but must be of higher potential difference. In some negatives this second rise in followed by a descent more rapid than that of the curve of discharge, and therefore indicating a small positive Acting P.D. I first noticed and called attention to it in connection with the curves illustrating my paper† on the "Time Relations of the Capillary Electrometer," but refrained from discussing its physiological significance.

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* 'Journal of Physiology,' vol. 18, p. 117.

† 'Phil. Trans.,' A, vol. 183, p. 104.